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may appear, is by no means satisfactory. The writer of the letter, in which he *may* have enclosed a stamp, though this is supposing an extreme case, receiving no answer, feels himself aggrieved, and writes again; so that in the end the receiver is forced to answer to protect himself. Is there, then, no remedy? Perhaps not. We nevertheless appeal to the public to bear in mind that the college-professor, however little he may have to do (and it is well known that this is very little), has at least something to do besides answering every question regarding business-matters in which it is thought that his advice may be of aid. Ask him any thing you please in the interests of matters pertaining to education or pure science, but draw the line when it comes to asking for what may fairly be called 'professional advice,' in the sense in which that expression is used by the lawyer and the doctor.

Two of the most unexpected discoveries in the deep-sea soundings during the last campaign of the *Talisman*, under the supervision of Prof. A. Milne-Edwards, are, first, the discovery of polished and scratched pebbles at a depth of five thousand metres, between the Azores Islands and the coast of France, indicating plainly the existence there of icebergs during the glacial epoch; and, second, of stones with impressions of parts of trilobites also brought up by the trawls. If these rocks with trilobites belonged where found, it will go far to prove the existence of an Atlantis continent during the secondary and tertiary epochs.

As a rule, one would not expect scientific knowledge to be much advanced, or very usefully diffused, by elegant extracts and quotations. But in a small book just issued by Appleton & Co., made up of 'characteristic passages from the writings of Charles Darwin,' Mr. Nathan Sheppard has really produced, in a form at once authentic, brief, and inexpensive, an instructive and very readable account of Darwinian doctrine in the words of its founder. The pieces are put together with no small

skill, not in the order of publication, but rather in the order of evolution. It begins with the movements and habits of plants, rises from these to worms, discourses of the variation and struggle for existence of the higher living forms, and so to the highest, —

'The diapason closing full in man.'

LETTERS TO THE EDITOR.

. Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The relations of *Didymodus*, or *Diplodus*.

MY reverence for the genius of Professor Cope is so great, and my confidence in his acumen so implicit, that when he assured me, first personally, and then in *Science* (iii. 275), that *Didymodus* (a substitute for *Diplodus*) was the proper name for *Chlamydoselachus*, I was willing to at least concede that the two forms might possibly be related. Knowing, as I did, that different types had been confounded under the name *Diplodus*, I was content to await the publication of Professor Cope's views before expressing a positive opinion, thinking he might have evidence in reserve which would gainsay what had been before offered. A *résumé* of Professor Cope's observations has just appeared, as promised, in the *American naturalist* for April (xviii. 412, 413), and we are therefore in a position to test his utterances. Notwithstanding the reverence and confidence that I have expressed, I can but think now, that for once Professor Cope has been too hasty, and tripped. I am convinced, not only that *Didymodus* has no generic nor even family relations with *Chlamydoselachus*, but that it represents even a different order. My belief in Professor Cope's candor equals my other sentiments, and I presume he will discard his first-formed opinion when his attention is called to certain facts.

The history of *Didymodus*, or *Diplodus*, is a long one, and is complicated with that of several others. I need only give the salient features.

In 1837 Professor Agassiz (*Poiss. foss.*, iii. 66) described a spine which he believed to have belonged to a fish like the sting-rays, as *Pleuracanthus laevisimus*. The only example was obtained from the Dudley coal-field.

In 1845 Professor Agassiz (*Poiss. foss.*, iii. 204) made known certain teeth, which he referred to sharks of the family of *Hybodonts*. Two 'species' were distinguished, *D. gibbosus* and *D. minutus*. Both were obtained from the English coal-measures.

In 1848 Professor Beyrich (*Berichte verhandl. k. preuss. akad. wiss.*, 1848) proposed the generic name *Xenacanthus* for a German carboniferous form referred to *Orthacanthus* by Goldfuss (1847), but which approached nearer to *Pleuracanthus*.

In 1849 Dr. Jordan (*Jahrbuch für min. u. geol.*, p. 843) described, under the name *Triodus sessilis*, a form subsequently ascertained to be identical with the *Xenacanthus*.

In 1857 Sir Philip de Malpas Grey Egerton (*Ann. and mag. nat. hist.*, xx. 423) contended that the spines of *Pleuracanthus* belonged to the same fish as the *Diplodus* teeth, and that *Xenacanthus* was likewise referable to the same type.

In 1867 Professor Kner (*Sitzb. k. akad. wiss.*, iv. 540-584) published an elaborate memoir, illustrated by ten plates, in which he proved conclusively

that Diplodus and Xenacanthus were generically identical.

In 1883 Professor Cope (*Proc. acad. nat. sc. Philad.*, p. 108) substituted the name Didymodus for Diplodus, because the latter name had been given in 1810 to Sargus by Rafinesque. The distinguished naturalist was evidently unacquainted with the researches of his predecessors.

There is much variation in the dentition of Pleuracanthus (as we shall now call Diplodus, or Didymodus), but it is rather a variation consequent on position in the jaws than specific or generic; and not only 'the species,' but one and the same species, may 'possess two, three, or four denticles,' but not teeth at all like Chlamydoselachus. However, somewhat analogous teeth are those of the type named Diplodus incurvus by Professors Newberry and Worthen (*Pal. Ill.*, vol. ii. p. 62, pl. 4, f. 4). These were very different from Diplodus, and belonged to a genus called Thrinacodus by St. John and Worthen (*Pal. Ill.*, vol. iii. p. 289, pl. 5, f. 1, 2). But whether the animals armed with such teeth resembled Chlamydoselachus may well be doubted.

In fine, the order called Ichthyotomi by Professor Cope appears to be demanded; but it has nothing whatever to do with the Pternodonta or Selachophichthyoidi, and it may not even belong to the selachians (some of its characters are very peculiar, and resemble those of protodipnoans). Further, the order had already been recognized, defined, and named by Lütken. Didymodus, or Diplodus, and Triodus, can be co-ordinated with the spines, Pleuracanthus, Orthacanthus (pt.), and Xenacanthus. All these names are referable to a single family (Pleuracanthidae) of the order Xenacanthini of Lütken. The proposed memoir of Professor Cope will, however, be a great boon to science; and to enable him to co-ordinate his data with those of the earlier paleichthyologists, and thus render it still more valuable, is the object of this communication. Apparently two genera, distinguished by their spines, exhibit the Didymodus, or Diplodus, dentition, — Pleuracanthus and Xenacanthus. Information is especially desirable respecting the character of their branchial apertures.

As to Chlamydoselachus, the anatomy will probably reveal a structure most like that of the Opistharthiri (Notidanidae), but of a somewhat more primitive type. Mr. Garman's memoir will unquestionably be of great value, for probably no one is better acquainted with the selachians than that gentleman.

THEO. GILL.

The 'unit of time' controversy.

Upon reading your editorial comments in *Science*, No. 58, upon the 'change in the unit of time' controversy, which close with the words "Unless, then, this matter admits of speedy and permanent decision, the one way or the other, with the entire agreement of all parties to the controversy, astronomy would appear to run the serious risk of forfeiting her claim to a place among the exact sciences," it strikes me, that unless the whole thing is intended as a sarcastic criticism of Mr. Stone, of which there is no evidence, it is about time to call a halt upon some one for loose writing.

If Mr. Stone maintains that a mean solar day, instead of depending upon the actual time of rotation of the earth on its axis and the actual time of its revolution round the sun (and hence capable of determination by actual observation), is an arbitrary interval of time fixed by the dictum (of Bessel, Leverrier, or any other human being) that in that time the earth shall move so far in its journey round the

sun (and that is exactly what his theory amounts to), and if he says,¹ "Professor Adams's argument, that 'mean solar time is measured, not by the sun's mean motion in longitude, as Mr. Stone's theory supposes, but by the motion of the sun in hour-angle,' is one that I do not profess to understand," and if he persists in maintaining these absurd positions, then astronomers will simply leave him to himself, for argument in such a case is useless.

As to the relation of astronomy to the exact sciences, let us see how much is the point in dispute. The increasing discrepancy between the formulae of Bessel and Leverrier for the annual mean motion of the sun in longitude is 0''.0602 per year; that is, six-hundredths of a second of arc while the sun moves 1,296,028 seconds. This amounts to eight-hundredths of a second of time (0s.08) in *twenty years*. Expressed as a ratio to the whole constant, it is .000,000,046, or about 1 part in 21,500,000. The discrepancy between the two best modern determinations — those of Hansen and Leverrier — is only 0''.0043 per year, or about one-fourteenth of the above; and perhaps it will be admitted by even the most enthusiastic devotees of the 'exact sciences' that this is a fairly well determined astronomical constant. The proper theme for exciting astonishment should be, that Bessel, with the data available in his day, should have been able to determine this, and a dozen other constants, so wonderfully near their true values as modern observations show them to be. Only an intellectual giant of his wonderful skill and indomitable energy could have accomplished such results.

H. M. PAUL.

Washington.

[*Caeteris paribus*, loose writing is much less probable than loose reading. We counsel our correspondent to re-read, and with circumspection. *Science* hopes to present the views of all parties when so expressed as to merit a hearing, and, least of all, takes occasion to espouse the cause of a partisan. The controversy on 'the unit of time' is regrettable; but foreign astronomers are abundantly competent to conduct the discussion, as they have done heretofore, without additions to the literature of the subject on the part of any one here.]

The use of the method of limits in mathematical teaching.

Science for March 14 contains a letter by Professor Safford on methods of teaching the calculus, in which he refers to the 'new method of rates' by the writers, in comparison with the method of limits. The phrase, 'new method of rates,' is quoted from a list of subjects for discussion by the M. P. club, Boston, and was probably intended as an abbreviation of the title of a pamphlet, "On a new method of obtaining the differentials of functions, with especial reference to the Newtonian conception of rates or velocities."

We have more recently published a treatise on the differential calculus, founded upon the method of rates or fluxions, in which the method published in the pamphlet is employed in obtaining the differentials of functions, but which has nothing in common with the methods used by Maclaurin, except the employment of the conception of velocity in the fundamental definitions.

Professor Safford regards the doctrine of 'the survival of the fittest' as having pronounced against the method of fluxions, and in favor of the method of limits. It seems to us that it is rather the *geometrical methods* of Maclaurin and the immediate followers of Newton that have thus been condemned, as com-

¹ *Monthly notices*, January, 1884, p. 81.